Fazekas

October 30, 1957

Dear Stephen:

This will probably be myx last communication to you from Australia, as we are on the point of leaving Melbourne. We are returning home westward, with stopovers of a few days each at Adelaide, Perth, Calcutta, Bombay, Milan, Geneva, New York and home. (A most unlikely itinerary in terms of choice of cities, but that comes of mixing business and pleasure.) We'll be home about Nov. 23; the Bact. Dept. here will forward any mail arriving meanwhile.

Unfortunately, we will just miss Frank Fenner in Madison by a day or two.

I particularly want to thank you for your exuberant generosity in sending the 'kit' for the shell-bits. That CARE parcel was just what was needed to encourage Burnet to take up the technique for his own work, and I sat in on that as well. Having overcome his initial inertia about working in a new method, he is very pleased with it indeed, and has already begun to use it in its obvious applications for genetic work (isolation of clones etc.)

I had a most instructive time in learning the virus techniques from Miss Larkin, and in reading a tremendous pile of papers— not yet all assimilated. I did not get very much done on incomplete virus; your own proposals still stand up as well as any— but more obviously has to be done on the effect of added mucoids. In fact, in having tried to set up some similar experiments myself, I ran into the very potent inhibition of growth(of PR8 or WSE) by allantoic fluid in the DEE, and the same held for other mucins. The inhibition was so marked that I did not have enough residual virus for testing the infectivity ratio. (One could I think argue that the increased infectivity you found might be the result of an effective reduction of multiplicity of infection.)

The finding of this inhibition (which, supposedly, does not hold for the whole egg) suggested its application for the selective isolation of certain recombinants. We have, for example, crossed MEL x WSE in DEE inoculated the first harvest in fresh DEE, washed these, and introduce M anti-MEL serum plus sheep mucin (± periodation). We have had great trouble in making a cleam selection, probably on account of phenotyp mixing, but did in one experiment get a pure yield of WS- recombi so I think there is something in the method.

This work should be followed up, but for the rest of my tir Melbourne I was diverted by the development of a technique fo characterizing the antibody output from single cells in lymp node suspensions. We've reached the point of validating th method; Gus Nossal is following that up immediately to se whether one cell produces one or many species of antibody.

I most emphatically want to remind you of yourppromise to keep me informed of your itinerary across the US, so that we will not miss an opportunity of seeing you.

Till then, with best regards to your wife,

Yours,

Joshua Lederberg

P.S. Of the material that Dr. White left here, I've abstratted two plastic trays and one separator, and sent these home. We will find the means to return the favor. The rest is left at the Hall Institute, and I trust that SirMac will return them as soon as convenient-- i.e. when he has obtained his own replacements, or worked out how to adapt his present trays.

By the way, your ms. referred to 'acridine plastics': is that right, or did you mean 'acrylic'?

When you do write up your statistical analysis-- egg variability etc-- I would be particularly grateful to be able to see a ms. copy, if that is convenient foryou.